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ABSTRACT

In this paper, the author offers an expanded explanation of just what difference sibling position should make, and then subjects this reasoning to a test that seems to meet all of the objections raised about empirical work linking sibling position to achievement. More specifically, the author (1) lays out a simple proximate explanation of the way in which family size, birth order, and child spacing should influence a child's subsequent attainments by governing the time and other inputs he or she receives; (2) transforms these three dimensions of family structure into a single quantitative family-inputs index; (3) tests the link between sibling position and achievement within families as well as across families; and (4) presents calculations suggesting the order of magnitude of the impact of improved contraception on the subsequent inequality and level of a cohort's educational and career attainments. It turns out that the link between sibling position and achievement is significant even when age and parents' attributes have been held constant. The impact of improved contraception on the distribution of achievements in the next generation turns out to be as important, or possibly more important, then would be inferred from past studies. The family-inputs index emerges as an important variable for future studies. (Author/JM)

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FAMILY INPUTS AND INEQUALITY
AMONG CHILDREN

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The research reported here was supported in part by the Institute for Research on Poverty, the Population Council and the Rockefeller Foundation. This paper is a modified version of Chapter 8 and Appendix C of a book in progress on Fertility and Inequality in America. The author wishes to thank Professor Kathryn E. Walker of Cornell University and Professor Albert I. Hermalin of the University of Michigan for supplying the survey data on time use and on New Jersey siblings, respectively. The expert programming assistance of John Cooper and Nancy Williamson is also gratefully acknowledged.

Reducing family size both reduces the inequality of earnings and raises their average level.

The evidence previously presented in support of this argument has been vulnerable to the charge that the omission of other variables correlated with both the numbers and the later achievements of children might have caused the importance of family size to be overstated. Some skepticism can be rejected on the basis of tests that took out the achievement effects of family size and sibling position were carefully. By examining the relationship of achievement differentials within families as well as differentials between adults raised in different families, this paper finds support for the argument that achievement is significantly retarded both for middle children in families of given size and for each child as the number of siblings rises.

Support is found for the hypothesis that one's sibling position not only matters for later schooling and careers, but also matters in the way one would have predicted by calculating how sibling position should affect the amount of time an individual sibling comes to receive from others. Predictions of the effect of sibling position on the time a child receives were generated from regressions using the 1967-1968 Cornell Time Use Survey. The same regressions yielded important side-results relating to the apparent effects of parents' education and mothers work on the time received by children.

Given that family size and birth order matter to later child achievements, one can calculate the impact of improved contraception on the

average level, and the inequality, of schooling attainment. It appears that more effective birth control promotes higher, and generally rather, the number of years of schooling. This schooling effect also benefits and raises earnings somewhat.

Confirming the intergenerational link between family size and later inequality underlines a potential dilemma for income-maintenance and welfare policies. If the generosity of family support affects fertility, society has reason to scale down the incentives to support that may tend to stratify; yet society also has reason to be generous in supporting the extra children once they are born.

to conform to the qualitative assertions just made. The other testing strategy is to quantify the predicted effect of sibling positions on the amounts of time and money that parents with a given set of attributes would be likely to provide for the development of a child in any given sibling position, and then to test the ability of this quantitative measure of family inputs to match the observed patterns of sibling achievement. This use of a quantitative measure of family inputs is a valuable check on whether the qualitative arguments in the literature stressing the importance of sibling position are right for a wrong reason.[9] Both kinds of tests are presented below.

Quantifying the above arguments about family inputs into a child requires using regression estimates from both time-use surveys and consumption surveys to calculate the expected effect of children in various sibling positions on family expenditures of time and money, given the attributes of the adults in the household. This procedure is complicated, time-consuming, and full of pitfalls, but reasonable estimates can be obtained. In the following discussion I shall focus mainly on the patterns of apparent time inputs into children and shall treat patterns of commodity inputs into children more briefly.

There are several difficulties in trying to use time-use surveys to infer how much time each child receives. One set of difficulties is that it is often hard to decide whether a given activity constitutes caring for or interacting with children. The reader can see the problem easily enough by asking himself whether in each of the following situations the parent is spending time on child care: parent and child are feeding the chickens together and chatting; they are watching a ball game together (on TV, or at the ballpark); Mother is ironing and watching the child across the room;

both parents are fretting in the child's absence about how they should handle the child's misbehavior. The problem of defining child care can be dealt with satisfactorily by limiting the scope of inquiry and by using the most careful survey data available. The arbitrariness of the time-use categories is less of a problem for present purposes than for some other inquiries because the biases built into the definitions of what constitutes child care are likely to be shared proportionally by families with varying ages and numbers of children. The problem of defining child care is also cut down by using a survey that is careful to gather a detailed account of time use over an actual day. This requirement is met by the Cornell time use survey described at greater length in the below appendix. The Cornell team gathered complete 24-hour time accounts for each of two days from 1296 Syracuse families in 1967 and 1968, thereby avoiding some of the sloppiness of response that is likely if respondents are simply asked how much time they spent on such-and-such an activity over a typical day or week. The Cornell team was also careful to give separate measurement to "primary" and to "secondary" time use, the former being the time spent on the activity that was the family member's main focus of attention and the latter being the time spent in more passive supervision or manipulation. This distinction allows one to omit from the calculation of time spent on a child any time spent on other activities while checking periodically, and without personal interaction, to be sure that the child is in no danger. By omitting such secondary child-care time from the calculations below, I have avoided the uninteresting finding that most young children receive <u>some</u> sort of supervision throughout their waking nonschool hours regardless of their sibling position or the nature of their parents.

The other main class of difficulties in interpreting time-use survey data is in allocating to an individual child a part of the total time reported as "child care." It is practically impossible for a survey to disentangle just how much of this time was received by an individual child when more than one child is present in the home. Only by thoroughly disrupting respondents' lives for an entire day could the interviewer see just how the total child-care time was divided up among siblings. In fact, some of it is not divided up at all, as when a parent reads a story to two children at once. In the face of these problems a precise measure of the time inputs received by one child simply cannot be obtained without adding theoretical assumptions. Two key assumptions that seem reasonable are:

(1) the amount of care time that an only child receives (outside of school) is a plausible upper-bound estimate of the amount received by a nononly child of the same age and parental attributes; and

(2) the net amount added to total child-care time by the presence of an extra sibling of given age and parental attributes is a plausible lower-bound estimate of the care time he receives--that is, he is assumed to receive (and to take from other children and from nonchild activities) at least as much time as the family transfers to all child care because of his presence.

These two assumptions have been made en route to the estimates presented below. A further assumption was made, however, to yield a single "best-guess" number for the time inputs into each sibling position. Since the truth lies somewhere in between the upper- and lower-bound estimates, I arbitrarily used the midpoint between these as the estimate of the time received by an individual nononly child. Readers may have hunches suggesting that this mid-range estimate is wrong. In its defense I can say only that it is used in regressions below in a way that (a) makes no difference as long as the lower-bound estimate exerts the same percentage effect

(whether or not the fifty percent assumed by the mid-range estimate) on true time inputs for all sibling positions, and (b) fits patterns of sibling achievement quite well.

Using the approach just outlined, one discovers certain patterns linking sibling position to the amount of time spent on child care. Even without the use of assumptions about the allocation of total child-care time among siblings, the regressions explaining the total amounts of child-care time (Appendix, Table A-1) underline the importance of the number and ages of children. The ages of children dominate differences in care time, with infants contributing far more to daily or annual hours of child care than children of school age. The number of children also interacts with their ages, with each extra child in a given age group adding significantly to the child-care load at a decreasing rate. These patterns hold for the use of the mother's time alone as well as for the total time contributed by all persons to caring for the children in one family.

Attributes of the parents prove much less important than family composition as determinants of the amounts of time spent in child care. The mother's age, her education, and the father's socioeconomic status all fail to exert statistically significant influences on the total amount of time spent (by the mother or by all persons) on caring for children of given number and ages. It appears that the only way in which more educated wives as a group manage to devote more time to each child is by having fewer children on the average.[10]

The importance of family composition even eclipses that of the mother's work status. It turns out that for given ages and numbers of children, mothers who work for pay outside of the home spend only slightly less time in child care than nonworking mothers. The differences by work status are

only near the border of statistical significance in the Cornell sample, though a much larger sample might make them clearly significant. Furthermore, for given family composition, children of working mothers receive more total time in child care from all persons. That is, the estimated deficit in mother's time is more than made up by extra (non-school) time inputs by the husband and by others.[11] Furthermore, if the numbers of children are not held constant, it turns out that working mothers, having fewer children on the average, spend as much of their own time per child on child care. Each child of a working mother also receives as much of others' time and greater amounts of commodities bought with the mother's pay.

If the number and ages of children are such a dominant influence on the amount of time devoted to child care, there is good reason to look more closely at estimates of how the amount of time going into an individual child is affected by the number and ages of his siblings. To make such estimates, one must make use of assumptions like those introduced above. In addition to deriving such mid-range estimates of the amount of time received by one child from each person each year, one must aggregate over caretakers and over years of the child's life in a way that requires further assumptions. I have opted for the simplest aggregation procedure, and have added up all of the hours of time received by a child (according to the mid-range estimates) from any person in any of the 18 years he spends in his parents' household. The total numbers of hours for a child in each of several sibling positions are given in Table 1, and illustrated in Figure 1. Readers should note, though, that the assumptions implicit in the simple toting of total hours of care received may not square with their own hunches

Table 1. Predicted Childcare Time Received over 18 Years by Children in Various Sibling Positions, as a Fraction of That Predicted for an Only Child, by Years of Spacing Between Adjacent Children (Based on 1967-69 data)

Child spacing (years)	Children ever born	Child's birth order				
		1st	2nd	3rd born	4th born	5th born
1-year	only child	8237 hrs. (1.000)	--	--	--	--
	2	6328 hrs. (.768)	4528 (.790)	--	--	--
	3	5328 (.647)	3993 (.696)	3570 (.672)	--	--
	4	5757 (.700)	3501 (.675)	3464 (.688)	3761 (.708)	--
2-year	2	7662 hrs. (.930)	6452 (.903)	--	--	--
	3	5640 (.685)	5481 (.844)	5698 (.893)	--	--
	4	5799 (.704)	5407 (.657)	5425 (.684)	5819 (.707)	--
3-year	2	6740 hrs. (.818)	6396 (.929)	--	--	--
	3	5853 (.711)	5668 (.865)	5867 (.719)	--	--
	4	5836 (.709)	5754 (.699)	5669 (.688)	6093 (.740)	--
4-year	2	7172 hrs. (.871)	7188 (.874)	--	--	--
	3	6138 (.745)	5669 (.733)	6026 (.809)	--	--
	4	6338 (.770)	5944 (.676)	5794 (.706)	6056 (.805)	--
	5	6339 (.770)	5964 (.674)	6059 (.695)	5796 (.706)	6056 (.805)

Notes:

The figure in parentheses is the ratio of the estimated time received by this child to that received by an only child.

Each estimate was derived by the following steps: (1) Regressions were run on the Cornell time use survey data, relating child care time by all persons to the

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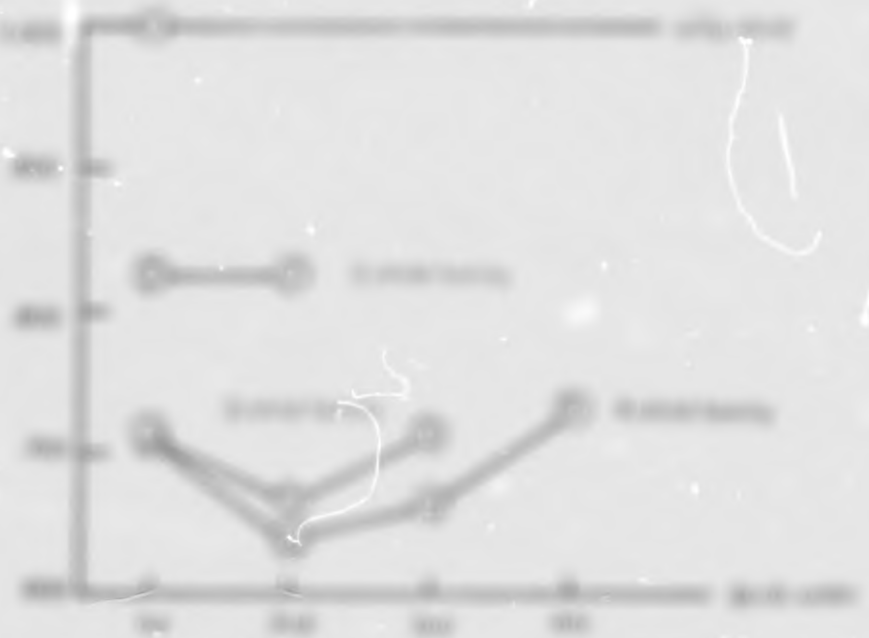
ages and numbers of children in the household and other variables. These regressions are presented in Appendix A. (2) For each child age-and-number variable, the coefficients from regressions for different parts of the cost (consumer expenditure, current workers, and unpaid work weighted by their shares of a total cost) were summed to get an annual coefficient. (3) The net impact of each extra child care hour in a given year was computed by adding and subtracting the coefficients just mentioned. For example, the net impact of a five-year-old with a one-year-old sibling would equal the coefficient for five year age group, two children in the lower price that the two-year-old, two children in the lower wage that the two-year-old, the only one in the home. (4) These net impacts are then summed over the 18 years that each child spends in the household. (5) The 18-year net impact total is converted to subtracting from that sibling's total impact on family child care the number of hours he or she spent taking care of younger siblings. This conversion is necessary to keep the paying child care supplied by an older sibling from being counted as time spent on his or others. The parameter estimates of such care for younger siblings by older are given in Table 3-3 of Appendix A. (6) As explained in the text, the estimated net impact of a given child on total care of family members is a lower-bound estimate of the hours of care time from which that child received some benefit. The upper-bound estimate is the hours of care time he would receive as an only child. As noted in the text, the next step is to calculate the difference between these two bounds. This "bit-range" estimate of what is presented in the table above. (7) For the global net impact figure, Willis Fix and Margie multiplied all 17 terms, the total care time for an only child.

For families with five or more children, dimmer estimates could not be made because too few such families were represented in the sample. In order to include large families in the econometric work on rates of time inputs varied, it was necessary to extrapolate values based on those for the largest families represented in the table. The rule for extrapolating was as follows: for all functions in Longer Families were the index for the functions in the largest family size represented here. For all two-couples use the two-parent's index for the largest family here, similarly extrapolate for cases of penultimate child order, and for all others we applied the rates for the second-born of the same income child spacing. The same rule could be used for the case of six-child families, but it is unlikely that such spacing would occur in families with six or more children.

[illegible]



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interview respondent is closely related to his sibling position, and in particular to his family size. In the absence of any other information about an individual in the sample, we know that the probability of his being one of the high-achievement respondent group is 1/N, where N is the number of siblings in his family. Thus all only children were interview respondents, as were half the siblings from two-child families, one-third of the siblings from three-child families, and so forth. Thus in this sample there is an unavoidable relationship between family size and respondent status, and another relationship between respondent status and predicted achievement.

The selection bias involved in the sample design can be handled in various ways. The range of options can be seen more clearly by referring to the causal relationships portrayed in Figure 2. The first option is the "reduced-form" strategy of arguing that the probability that an individual is the interview respondent for his family is itself simply one channel through which the true importance of sibling position for achievement shows up in this particular sample. Following this strategy, one would simply ignore who the respondents were, and run regressions explaining achievements in terms of the variables on the left in Part A of Figure 2: family background, sibling position, and sex. This procedure, of course, is likely to overestimate the importance of sibling position and sex. The interview respondents were not a random sample of males, nor were the only-child respondents a random sample of only children, or the two-sibling respondents a random sample of persons from two-child families, and so forth. Such a strategy will be tried out below, but with the expectation that it overstates the importance of sibling position.

Two other testing options are likely to attribute less, and probably too little, importance to sibling position as a determinant of achievement. One is the device of adding a dummy variable for respondent status to the regressions explaining educational and career attainments. This is likely to understate both the unit impact and the statistical significance of sibling position. By mixing the respondents into the sample, this procedure is biased against sibling position by implicit

tion was an influence in the likelihood of one's being a respondent in this survey, and by including a subsample (the respondents) selected by occupational class, within which any independent variable has diminished explanatory power. A better procedure is to run tests on the subsample of nonrespondents. This can keep the nature of the respondents from biasing the estimates of the impact of sibling position. Still, tests on a sample of siblings who are not respondents is a stern one for the sibling-position hypotheses, since it cuts down the representation of the small families over which family size was supposed to matter most. Tests from a sample of nonrespondents are thus likely to underestimate the significance (t-statistics) of sibling position variables.

The best way of dealing with the special nature of the sample, however, is to run the kind of test that is also best for meeting the suspicions about omitted family variables raised by the past literature: examine intrafamily differentials in achievement. Part B of Figure 2 sketches a set of influences that could be tested by redefining all variables as differentials between the first-born and a later-born, to be employed over a sample consisting of all of the later-borns (non-first-borns) in the New Jersey sibling sample. Explaining intrafamily differences in achievement should give the most accurate indication of the importance of sibling

position. The special nature of the group of interview respondents is less of a problem in an intrafamily test. Since family size is the same for each first-born as for each of his own siblings, a first-born and each later-born in the same family have the same likelihood of being the interview respondent working for the New Jersey utility (except to the extent that birth order truly affects achievement). Therefore regressions following the scheme of Part B in Figure 2 should reveal the role of sibling position with or without dummy variables identifying the interview respondents. These intrafamily tests, like the other testing options just mentioned, will be probed next.

Achievement Patterns: Regression Results

When all of the different ways of correcting for the special nature of the New Jersey sibling sample have been tried, it turns out that sibling position is at least as significant an influence on achievement as past literature implied, and for the simple reason that sharing family time and money with siblings is a drag on achievement. This conclusion emerges from results like those given in Tables 2 through 4. To survey these results, let us examine the three tables in order.

Whenever the schooling levels of individuals in different families are analyzed, as in Table 2, certain background variables prove unmistakably significant. Schooling levels are higher, the higher are the parents' schooling levels and occupational status. A broken-home background significantly reduces one's likely educational attainment. Sex and age, on the other hand, were not significant influences on schooling in this sample. Both sex and age looked significant only in those regressions where they were allowed to act as proxies for interview respondent status, the respondents being all male and all over 55 (cf. Regressions [1] and [4] in Table 2).

Table 2

Patterns of Individual Educational Attainment:
Regression Results from the New Jersey Sibling Sample.

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Definitions of Variables Used

Dependent variables:

ED = a one-digit schooling code, ranging in values from 1, for less than an 8th-grade education, to 8, for 5 or more years of college. (An alternative scaling was also tried: the ED code was transformed into a logarithm of the earning potential predicted over a lifetime (capitalized at 5 percent back to age 14) on the basis of this schooling code alone. The figures for earning potential are based on 1959 data for northern white males, taken from Glora Hanoch, "Personal Earnings and Investment in Schooling," unpublished Ph.D. dissertation, University of Chicago, 1965, Table 8. This alternative scaling yielded results essentially identical to those for the regressions reported here for ED.)

SEI= a two-digit index of socio-economic status, based on the individual's most recent occupation. The index number assignments were made by Professor Albert J. Hermalin.

Independent variables:

MALE = 1 if the individual is a male, 0 if female.
EDMA = the mother's educational attainment = ED for the mother.
EDPA = the father's educational attainment = ED for the father.
SEIPA = the socio-economic status of the father's final occupation = SEI for the father.
BROKEN= dummy variable for a broken home during the individual's childhood = 1 if the parents were separated or divorced or if the interview respondent spent time in a foster home or orphanage.
AGE = the individual's age in 1963, in years.
AGESQ = AGE squared.
RESP = 1 if this individual is the interview respondent, 0 if this is one of his siblings
SIBLOSS = index of reduction in time inputs received by the child because of his having siblings = one minus the index given for the appropriate sibling position in Table 1. The indices calculated for 1-year spacing in Table 1 were used whenever average child spacing was less than 1.5 years; the 2-year indices were used for spacing from 1.5 years to 2.5; the three-year figures for spacing from 2.5 to 4.5; and the 5-year figures for spacing over 4.5 years.
ONLY = 1 if the individual is an only child, 0 otherwise.
FIRST2, FIRST3, FIRST4&5, FIRST6&O each = 1 when the individual is the first-born of two, three, four or five, and six through thirteen children, respectively.
MID3, MID4&CLO, MID4&WID = 1 when the individual is an intermediate-born child of three children, of four or five children spaced more closely than two years apart, or of four or five children spaced two years or more apart, respectively.
LATTER, LAST3, LAST4&, LAST6&13 = 1 when the individual is the last born of two, three, four-or-five, or six or more children, respectively.
TWO, THREE, FOUR, FIVE = 1 when the number of siblings (including the respondent) is two, three, four, or five, respectively.
N = number of siblings (including the respondent).
NSQ = N squared.

Table 2 (cont.). Patterns of Individual Educational Attainment: Regression Results from the New Jersey Sibling Sample

Independent variable	Regression number (dependent variable is ED): (1) coeff.	(1) std.err.	(2) coeff.	(2) std.err.	(3) coeff.	(3) std.err.
constant term	-1.815	(2.268)	1.198	(2.351)	1.233	(2.343)
MALE	.298**	(.109)	.076	(.116)	.078	(.118)
EDMA	.349**	(.052)	.226**	(.060)	.251**	(.052)
EDPA	.223**	(.046)	.260**	(.053)	.223**	(.046)
SEIPA	.018**	(.003)	.018**	(.003)	.018**	(.003)
BROKEN	-.505**	(.172)	-.562**	(.201)	-.526**	(.170)
AGE	.138	(.078)	.035	(.081)	.033	(.081)
AGESQ	-.0016*	(.0007)	-.0005	(.0007)	-.0005	(.0007)
RESP	---	---	---	---	.636**	(.1401
STATUS	---	---	---	---	---	---
ONLY	1.671**	(.294)	---	---	.658*	(.305)
FIRST2	.742**	(.216)	.718*	(.316)	.512*	(.219)
FIRST3	.531*	(.258)	.533	(.283)	.410	(.237)
FIRST45	.209	(.217)	.248	(.232)	.178	(.215)
FIRST613	-.15*	(.269)	.423*	(.287)	.589*	(.267)
MID3	.413*	(.236)	.644*	(.263)	.551*	(.234)
MID4TO10	.086	(.250)	.007	(.264)	.111	(.248)
MID45ID	.250	(.176)	.151	(.193)	.194	(.175)
LATTER	.454	(.215)	.365	(.252)	.369	(.214)
LAST3	.436	(.238)	.346	(.282)	.329	(.237)
LAST45	.480	(.211)	.489	(.248)	.387	(.210)
LAST613	.455	(.234)	.300	(.304)	.315	(.175)
$R^2_{adj.}$/s.e.e.	.3074/1.5664		.3144/1.5334		.3210/1.5709	
degrees of freedom:	988		705		987	
Sample:	1008 persons		719 non-respondents		1008 persons	

* = significant at the 5 percent level.
** = significant at the 1 percent level.

Note: the coefficients on sibling-position linear variables show the impacts of these positions relative either to middle children in families of six or more [in regressions (1)-(3)] or to all children in families of six or more children [in regression (7)].

Table 2 (Cont.). Patterns of Individual Educational Attainment: Regression Results from the Two-Sex Pooled Sample

Independent variable	Regression number (dependent variable is EDU): (1) coeff.	(1) std.err.	(2) coeff.	(2) std.err.	(3) coeff.	(3) std.err.
constant term	-1.252	(2.134)	1.919	(2.162)	1.770	(2.146)
MALE	.302**	(.108)	.058	(.115)	.063	(.117)
EDMA	.250**	(.052)	.229**	(.060)	.253**	(.052)
EDFA	.213**	(.046)	.263**	(.053)	.224**	(.045)
OCCPA	.018**	(.003)	.018**	(.003)	.018**	(.003)
RANKIN	-.482**	(.170)	-.572**	(.190)	-.505**	(.168)
ACE	.152*	(.076)	.039	(.079)	.036	(.077)
ACESQ	-.0019*	(.0007)	-.0005	(.0008)	-.0006	(.0007)
RELP	---	---	---	---	.682**	(.157)
CONSTANT	-3.393**	(.617)	-2.736*	(.958)	-3.272*	(.638)
R^2_{adj} /s.e.e.	.3056/1.5689		.3128/1.5353		.3234/1.5705	
degrees of freedom:	999		710		998	
Sample:	1001 persons		719 non-respondents		1008 persons	

Independent variable	Regression number (dependent variable is ED): (1) coeff.	(1) std.err.	(2) coeff.	(2) std.err.	(3) coeff.	(3) std.err.
constant term	-.387	(2.161)	.121	(2.250)	.596	(2.210)
MALE	.380**	(.109)	.333**	(.108)	.363**	(.108)
EDMA	.251**	(.052)	.246**	(.052)	.251**	(.052)
EDFA	.228**	(.046)	.228**	(.046)	.216**	(.045)
OCCPA	.018**	(.003)	.017**	(.003)	.018**	(.003)
RANKIN	-.511**	(.172)	-.489**	(.171)	-.440**	(.171)
ACE	.093	(.076)	.095	(.077)	.092	(.077)
ACESQ	-.0018	(.0007)	-.0018	(.0007)	-.0018	(.0007)
RELP	---	---	---	---	---	---
CONSTANT	---	---	---	---	---	---
ONLY	.963**	(.291)	---	---	---	---
TWO	.444**	(.165)	---	---	---	---
THREE	.389*	(.154)	---	---	---	---
FOUR	.137	(.156)	---	---	---	---
FIVE	.189	(.155)	---	---	---	---
N	---	---	-.073**	(.022)	-.249**	(.075)
NSQ	---	---	---	---	.014*	(.006)
R^2_{adj}/s.e.e.	.3045/1.5899		.3025/1.5922		.3058/1.5886	
degrees of freedom:	995		999		998	
Sample:	1008 persons		1008 persons		1008 persons	

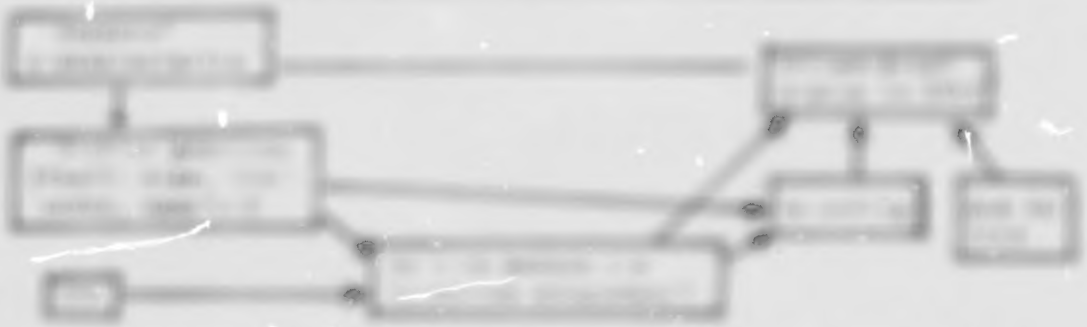
* - significant at the 5 percent level.
** - significant at the 1 percent level.

[illegible] of several specification products that present family size reduces the schooling expected for each sibling. The first three regressions also suggest that closed child spacing (the fewer and fewer-child families) may be a disadvantage for educational attainment, but the spacing patterns are not statistically significant. In the tenth order (the regression [1] - [10]), the hypothesized disadvantage of having a middle child shows up among families with more than three children, but fades in more sophisticated tests.

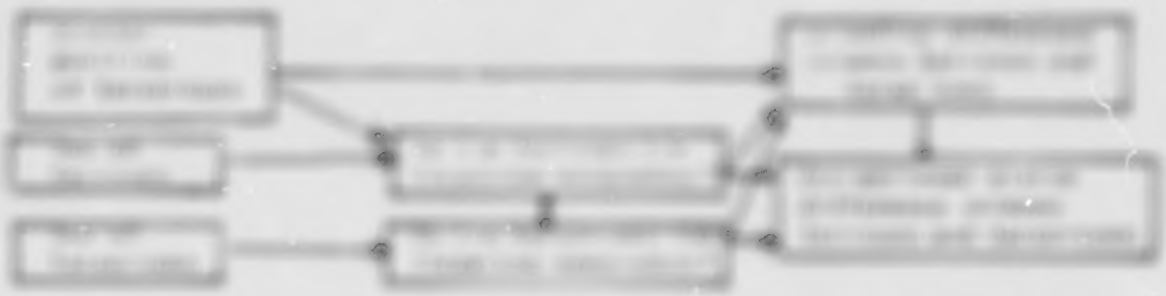
Accounting the sibling position variables into the simple SCHLYRS index (the regressions [4] through [6]) yields mixedly suggesting that the measure sibling positions contents is permanently because entire siblings orders may amount of time and money spent on each child. This suggestion comes from the ability of the SCHLYRS index to overestimate almost as much in explaining achievement variations as a dozen sibling-position dummy variables. The SCHLYRS index even remains significant at the five percent level when forced to compete with the respondent-relative dummy with which it is so highly (negatively) correlated. Its predictive power in all the even eventuality when one remembers that the children in the five-year orders whose ages underlies the SCHLYRS estimates are on the average half a year younger than the adults whose schooling levels SCHLYRS helps to explain. Table I, then, seems to support all of the sibling-position hypotheses.

All family background variables used to explain schooling levels better than they explain post-[illegible] occupational achievements. The reason is that school results intervene between the completion of schooling and the height of one's earnings. Accordingly, Table I shows that all background variables are less significant, and account for a smaller share of

[illegible]



[illegible]



broad generality, prior to undertaking primary research into that proposition. Not coincidentally, age has been used to be more consistent evidence than other early antecedents. Socioeconomic status is probably superior age as well, since the socioeconomic gross domestic fund a family headquarters occupational group. Sibling positions, like the other background variables,[20] do not characteristic significant as a job course determinant for high-school [illegible] and [illegible] of table 21, though its ramifications are still apparent at the "bi-yes" stage.

The base scales for assessing occupations given material variables and sample occupation titles and its scientific expressions involved in table 5. As as well as sought the explanations as occupations participant brilliant right against environment. Thirty, the past interviewer was interested in the suggestion that each one of persons who have had subsequent attributes that have their previous higher-university children, and their children, that other progress with similar behavioral attributes. If so, a longer number of children would seem to be a prudent step as achievement that is shifts to, given all emotional fluctuating motivations to several history of the per- sons. Second, as argued in the previous section, the nature of the two factors seems suggests that occupational level was most high with the prop- ber of sample attention-time.

One might expect the occupations level activities their dependencies exist by different men for the difficultation backfields to years. That the dependent variable is a difference in the schooling of the siblings natural superiors, R^2 is going to be low. The dependent variable is a simple fight, and one of the best possible discriminants of achievement across [illegible]-systems' education and occupational rights, a tremendous training, and even employment table plan-set of as help to accounting the socioeconomic

suggests that the most important omitted characteristic may be child-orientation. That is, it may be that within each educational and occupational class individual couples having more children may also be more effective at producing highly schooled, high-status children because their aspirations and/or talents center more on child-rearing. If so, this would explain why the tests comparing individuals from different families attached a lower impact to sibling position: holding parents' education and status constant, being from a larger family appeared to have only moderate influence on achievement when in fact its larger influence was partly offset by a tendency of certain parents with larger families to have other qualities raising achievement.

The omitted-variable bias suspected for the New Jersey sibling sample, however, may not exist for other samples. Here lies a possible resolution of the issue whether the effect of family size tapers off after the third child. The New Jersey sibling sample consists of higher-status individuals with high-status parents, on the average, than are picked up in such samples as the 1962 Survey of Occupational Change in a Generation used by Beverly Duncan. It could well be that the larger families in the OCG sample differ from each other in a way that is the opposite of the pattern just hypothesized for the New Jersey sibling sample. The larger families of the OCG sample are lower in average status and presumably characterized by greater variation in the effectiveness of birth control and foresightedness. Thus it seems more likely for the OCG sample than for the New Jersey sample that parents of eight or nine children differed from parents of four children in unmeasured qualities correlated positively with the number of children and negatively with average achievement of the children included in the sample. In the higher-status New Jersey sample, on the other hand,

it seems more reasonable to hypothesize that the extra children in large families were planned, as were future child resources, and that the main unobserved parental trait was a degree of child-orientation. This reasoning might reconcile the tendency of extra siblings to reduce average achievement more over the large-family ranges of the CCG sample than in the large families of the New Jersey sample.

The main outcome of the tests, then, is that sibling position is indeed a significant influence on schooling, and when other things have been held as constant as possible, its influence is in fact greater than ordinary testing methods would suggest. Being from a larger family and being a middle sibling are important achievement disadvantages on the average.

Birth Control and Unequal Schooling Attainment

If sibling position matters to schooling and career attainments, the decline in American birth rates must have made some contribution to reducing inequalities of schooling, status, and income. To know how great a contribution it has made, one must decide on an appropriate hypothetical experiment. The experiment involves imagining how a given cohort would have been distributed over different completed family sizes if births were hypothetically restricted. That in turn requires knowledge of how both achievements and the incidence of prevented births are spread over different sibling positions.

To get a rough idea of the degree of impact of birth restriction on inequality in schooling, let us ask: How does the variability of schooling among the 1008 individuals in the New Jersey sibling sample seem to compare with the variability of schooling in the hypothetical subset of those individuals out of the 1008 who would have been born under conditions of

more effective birth control? To imagine the situation with more effective birth control, let us first divide the sample of 1008 individuals into each birth order and each of three classes based on the mother's schooling: less than high school graduation, high school grad, and some college. For each birth-order-and-mother's-schooling group, let us assume that the percentage of births prevented is the percentage of such births between 1960 and 1965 found to have been unwanted in the 1965 National Fertility Survey (Bumpass and Westoff, 1970, p. 1179). This is presumably an underestimate of the incidence of unwanted births among those surviving in the New Jersey sample, since contraception was less effective in the prewar cohorts represented in the sample than in the early pill years investigated in the 1965 survey. The hypothetically prevented unwanted births turn out to be about 21 percent of the total (23 percent for mothers in the lowest-schooling group, and 14 percent for mothers with some college). Let us next regroup the 798 wanted births into their new family size groups. Finally, we compare the standard deviations and means of the schooling of these 798 with the original 1008, under the assumption that the means and variances within each family-size-and-mother's education group is unchanged. The result is a measure of how the average level and variability of schooling would be affected if nothing changed but the numbers of persons falling in each group.

The impact of the hypothetical birth restriction is unimpressive according to some measures but more noteworthy according to others. The mean schooling index of the sample of 798 wanted births was 3.57 percent, or roughly 0.32 years, higher than that of the sample of all births.[19] The standard deviations hardly differed at all in absolute terms, being only 0.3 percent lower with the improved birth control. At first glance it

appears that birth reduction could have raised average schooling noticeably while leaving the inequality of schooling unaffected. Yet for several reasons this appearance understates the contribution of birth reduction to the historic leveling of American educational attainment. One reason is that the level of the standard deviation is raised by the higher average schooling of the wanted-birth group. When the standard deviation is expressed as a share of mean years of schooling, it proves to be 3.74 percent lower with the smaller-family group. That represents a minor but noteworthy contribution to the total reduction in schooling inequality over successive twentieth-century cohorts, especially if one compares the New Jersey respondents' birth cohort (1905-1911) with the cohort of 30 years later, which had a fertility rate that was about 37 percent lower, instead of the 21 percent used here.[20]

The impact of birth reduction on schooling inequality would also appear larger in a perspective spanning two or more generations. The higher average schooling, and slightly less variable schooling, of a generation born into smaller families would not only transmit itself to the following generation for constant family size, but would also be a force reducing fertility further in the next generation, thereby making the cycle of fewer births and higher and more equal schooling become cumulative.

It should also be remembered that this estimation of the impact of birth restriction takes as fixed a reward structure that birth restriction apparently changes. A drop in the birth rate will slow down the growth of the labor force, thereby raising unskilled wages relative to top salaries and profit rates. For this reason, too, birth restriction tends to reduce inequalities in income and schooling.

There is, then, sufficient reason to conclude that fertility rates have a noteworthy effect on the levels of inequality of schooling (and incomes) in America. The desire to promote greater equality of income is thus one more justification for antinatalist policies. This is not to say that the glacial progress toward equality through birth restriction is fast enough. To take great comfort in the equalizing effect of birth rate decline over the generations, one would have to attach less urgency to the goal of more equal schooling and incomes than is professed by most Americans.

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Notes

1. Throughout this paper, as in most of the literature on this subject, the term "achievement" is used as a shorthand for educational and career attainments. The measure of educational attainment is simply the number of years of schooling completed by some adult age, or some index monotonically related to the number of years of schooling. The measure of career attainment available in the relevant data samples is an index of the socioeconomic status of the person's occupation. Lifetime potential income would also be a reasonable measure of career attainment for present purposes if it were available. The term "achievement" is not meant to apply, however, to IQ, grades in school, emotional stability, or happiness.

2. Some recent studies focusing at least in part on the influence of family on achievement attainment are: Adams and Nelson (1968); Bayer (1968, 1969a, 1969b); Blau and Duncan (1967); and Tyler (1971, pp. 423-452). Studies briefly noting family size as one of many influences on educational and/or career attainments are: Bowles (1970); Burke (1972); Beverly Duncan (1965); Otis Dudley Duncan (1967); Duncan, Featherman, and Duncan (1972); Johnson and Stafford (1973); and Leibowitz (1974).

3. See Alexander (1968); Bayer (1969, 1969a, 1969b); Blau and Duncan (1967); Schaefer (1968); and Turner-Smith and Rosenberg (1970, Ch. 3); and the sources cited there.

4. See Bayer (1968, 1969, 1969b), and Blau and Duncan (1967).

5. See, for example, Adams and Nelson (1968); Alexander (1968), the exchange between Portes (1967) and Bayer (1969b), Sorensen (1969), Sewell and Stewart (1968), and Turner-Smith and Rosenberg (1970, Ch. 3).

6. In what follows, I shall use the term "sibling position" as a phrase encompassing family size, birth order, and spacing between siblings.

7. In her handling of the same survey data, Beverly Duncan (1967) did examine each five-year age group of respondents separately, again finding a consistently negative predicted effect of number of siblings on educational attainment. She did not present data on the statistical significance of this effect, however, and confined her attention to the number of siblings rather than exploring the effects of sibling position and child spacing.

8. For a convenient review of the relevant literature, see Turner-Smith and Rosenberg (1970).

9. A natural objection at this point is that the quality of adult-child interaction may be a more important influence on achievement than the amounts of time and money spent. That is possible, but the drift of the recent literature is that the hypothesized effects of sibling position on both the quantity and the quality of family support for the child are quite similar. The existence of extra siblings in the home is supposed to lower

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the quality of parent-child interaction roughly in proportion to the number of the amount of time and commodities invested in the child. Thus, to test the most quantifiable part of the hypotheses about sibling position is to test the entire idea of resources.

This does not mean that the quality and quantity of support for a child's development are proportional in all families. If the subject of investigation were parental attributes rather than sibling position, more direct attention would have to be given to the quality of interaction that is given here. More educated parents, for example, may well be more "productive" at combining their time and financial resources to produce high-education and high-stature children than are less educated parents with the same income. This is very possible even though they apparently do not spend significantly more time interacting with their children than do less educated parents.

10. This finding contradicts the conclusions of other authors (e.g., Hill and Stafford [1972], Leibowitz [1973]) that more educated mothers spend more time with their children. My reasons for concluding that they have based this finding on inappropriately measured variables are given in Appendix C.

11. For an examination of the patterns of child-care time by mother's work status, see Appendix C. A calculations based on the case of a two-child family with three-year spacing suggests that the extra amount of care time received by the children of a working mother from persons other than her is half again as large as the loss in time inputs from her because she works outside the home. This implies that the children of the working mother are receiving less quality-adjusted time inputs in all main. If one could somehow allow that as kind of the mother's time contributions more to the child's development even at the margin than 1.5 hours of the time of others caring for her children. It seems unlikely that a strong case could be made for an error in perception in achievement contributions at the margin.

12. Appendix D of Family and Society in Sweden. By 1977 it should be possible to make more direct calculations of consumption effects of children, by drawing on the greatest detail for expenditures and for inputs on the ages of children to be made available by the 1974-75 Consumer Expenditure Survey.

13. One piece of additional evidence that tends to support this conjecture is a set of calculations I made of a proxy for the relationship between sibling position and the individual child's commodity inputs. I calculated the net impact of a sibling in each position on total family consumption over the 18 years of his childhood into which this is not that the child received, but just his impact on family expenditures. Each index used to allocate total family consumption were strongly true do first-born or lap-specialist middle-born children. It seems likely that the size amount of commodities being invested in a last-born is also larger than that invested in each of his older siblings. The proxy was also enough higher for the last-born to suggest that his input advantage over earlier siblings was greater for commodity inputs than it was for time inputs. It thus appears that within each family also the last-borns are valued in a more commodity-intensive fashion than their older siblings.

[illegible] [illegible] [illegible] [

discarded. The net results of this sample design procedure were a sample of 925 nonsummer weekdays, a sample of 734 weekend days, and a sample of 362 summer weekdays.

Since the focus of this study is on the changes in behavior associated with extra children, it is important to specify the family composition variables carefully. Children must be broken down into different age groups, since a newborn requires much more time inputs than a teenager. It is also likely (as the results below confirm) that a child of given age has less effect on the parents' use of time the more siblings there are in the home. There is thus a need for presence-of-children variables that count the number of children in a particular age group within families of a given total number of children, e.g., there should be a variable counting the number of children aged 6-11 within families having four children in the home. This precaution is taken in the regression models below, and the results confirm that a 6-11 year old child has less effect on family time use, and appears to receive less attention, in a family of four children than when he is an only child of the same age.

Time Use Regression Results

Table A-1 displays selected regressions showing the effects of children in the home and other variables on the amounts of time given to various home tasks. Long as the list of independent variables may be, some variables are not displayed here. Table A-1 leaves out the NKIDS coefficients and standard errors for families having more than four children (NKIDS50 through NKIDS74). These were included in the regressions, but are omitted here because some of them proved less reliable than the NKIDS coefficients for families with four or fewer children. Specifically, it turned out that the

Definition of Variables Used

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Characteristics of Husband and Wife

PREDWAGE = predicted hourly wage rate of the husband in 1969, in dollars per hour. The wage rate is a transformation on the educational attainment, occupational class, and exact age of the husband, using the detailed table of predicted values given in Boone A. Turchi, "The Demand for Children: An Economic Analysis of Fertility in the United States," unpublished doctoral dissertation, University of Michigan, Ann Arbor, 1973, Table VII-1.

COLLEGEW = 1 if wife has received a college degree, zero otherwise.

HSW = 1 if wife graduated from high school but does not hold a college degree, zero otherwise.

AGEWIFE1 = 1 if wife is under 35, zero otherwise.

AGEWIFE2 = 1 if wife is 35 but less than 45, zero otherwise.

AGEWIFE3 = 1 if wife is 45 but less than 55, zero otherwise.

DISABLEW = 1 if wife is ill or handicapped on the day of the interview data, otherwise = 0.

Date of Interview

SUMMER = 1 if the interview data refer to a date in June, July, or August, zero otherwise.

SUNHOL = 1 if interview refers to a Sunday, zero otherwise. Due to a coding error, about half of the Sunday observations were incorrectly recorded as non-Sunday observations. There is no known relationship between the incidence of this error and other independent variables, so that its only effect appears to be to cast doubt on the SUNHOL coefficient itself within the samples confined to weekend observations.

Time Spent on Various Kinds of Household Work (all variables in min./da.)

DRUDGEW = time spent by wife on all household work.
DRUDGEH = " " " husband on all household work.
DRUDGE27 = " " " children 12-17 on all household work.
DRUDGE61 = " " " children 6-11 on " " " .
DRUDGES = total time spent on all household work by all persons.
CARE1W = time spent on physical care of family members by the wife.
CARE1H = " " " " " " " " husband.
CARE1S = " " " " " " " " all persons.
CARE2W = " " other " " " " " the wife.
CARE2H = " " " " " " " " the husband.
CARE2S = " " " " " " " " all persons.

Numbers and Ages of Children in the Home

NUMKIDS = number of children in the home.
KIDS0 = " " " " under 1 year in age.
KIDS1 = " " " " 1 year old.
KIDS2 = " " " " 2-5 years old.
KIDS3 = " " " " 6-11 years old.
KIDS4 = " " " " 12-17 years old. (None in the sample were 18 or older.)

NKIDSij: the number of children in a family with i children who are in the jth age group. That is, the NKIDSij variables are a transformation on the total number of children (NUMKIDS) and the number in each age group (KIDSj) as follows:

Value of NKIDSij = Value of:	Total number of children (NUMKIDS): 1	2	3	4	5	6	7
KIDS0 :	NKIDS10	NKIDS20	..	..	..	..	NKIDS70
KIDS1 :	NKIDS11						..
KIDS2 :	NKIDS12						..
KIDS3 :	NKIDS13						..
KIDS4 :	NKIDS14	...	..	..	..	..	NKIDS74

(Thus, for example, a family of four children aged 7, 5, 3, and 1 would have NKIDS43 = 1 (the 7-year-old), NKIDS42 = 2 (the two middle children), NKIDS41 = 1, and all other NKIDSij variables = 0.)

TABLE A-1

Selected Home Time Use Regressions, Syracuse Survey, 1967-68
(Dependent variables in minutes per day)
(Standard errors of regression coefficients in parentheses)

	Dependent Variable:			
Independent Variables:	(1) DRUDGEW: wife's total home work time	(2) DRUDGEW: wife's total home work time	(3) DRUDGEW: wife's total home work time	(4) DRUDGES: total home work time by all persons
CONSTANT	247.6	228.4	258.8	368.8
	(37.1)	(87.1)	(47.5)	(51.0)
COLLEGEW	-17.6	.058	23.7	.0
	(22.6)	(38.5)	(27.1)	(31.1)
HSW	22.6	14.1	-15.6	13.6
	(18.0)	(27.9)	(21.2)	(24.7)
PREDWAGE	10.0	5.5	6.9	5.5
	(4.9)	(10.3)	(6.2)	(6.8)
AGEWIFE1	-37.0	71.0	-7.7	-98.4
	(32.1)	(81.2)	(41.5)	(44.2)
AGEWIFE2	-19.5	57.9	-6.7	-76.6
	(32.9)	(82.3)	(42.4)	(45.2)
AGEWIFE3	6.9	104.7	25.7	-42.2
	(32.0)	(82.2)	(42.1)	(44.0)
DISABLEW	-20.1	226.8	-6.4	63.9
	(42.0)	(79.4)	(58.7)	(57.9)
NKIDS10	287.9	182.6	150.3	349.9
	(32.7)	(56.1)	(38.6)	(45.0)
NKIDS11	241.9	148.9	97.5	296.4
	(31.8)	(62.4)	(38.7)	(43.7)
NKIDS12	101.2	-34.8	59.9	172.8
	(32.3)	(58.8)	(38.5)	(44.4)
NKIDS13	108.4	116.6	58.2	178.8
	(31.3)	(63.9)	(38.2)	(43.1)
NKIDS14	57.7	6.1	56.8	166.4
	(32.3)	(60.8)	(38.3)	(44.5)
NKIDS20	184.7	181.8	133.0	217.6
	(22.7)	(37.3)	(27.0)	(31.2)
NKIDS21	161.4	145.0	109.8	203.6
	(22.7)	(34.7)	(26.0)	(31.3)
NKIDS22	122.1	83.6	82.7	166.6
	(13.9)	(24.4)	(16.6)	(19.1)
NKIDS23	92.5	59.6	58.0	148.9
	(13.7)	(26.4)	(16.6)	(18.8)
NKIDS24	36.1	13.2	45.2	96.1
	(14.7)	(28.5)	(18.2)	(20.3)
NKIDS30	106.9	223.5	177.7	125.2
	(31.4)	(56.0)	(42.2)	(43.3)
NKIDS31	151.5	140.4	93.6	140.8
	(27.6)	(44.0)	(30.0)	(38.0)

TABLE A-1 (cont.)

Independent Variables:	Dependent Variable: (1) DHUSWE: wife's total home work time	(2) DHUSWEW: wife's total home work time	(3) DHUSWEW: wife's total home work time	(4) DHUSWES: total home work time by all persons
SKID632	101.9	45.5	44.1	125.3
	(13.1)	(22.2)	(15.4)	(18.1)
SKID633	48.9	46.0	33.5	121.1
	(9.4)	(15.4)	(10.9)	(12.9)
SKID634	20.3	5.6	16.9	93.1
	(9.4)	(17.3)	(11.8)	(13.6)
SKID640	144.1	48.4	107.3	241.1
	(31.9)	(51.8)	(36.4)	(43.9)
SKID641	123.8	93.1	35.1	136.7
	(33.7)	(50.7)	(33.3)	(44.4)
SKID642	86.1	53.5	42.2	91.6
	(11.5)	(17.9)	(23.5)	(15.8)
SKID643	55.4	31.1	30.4	104.4
	(7.8)	(14.2)	(9.3)	(10.4)
SKID644	37.5	10.6	24.9	53.9
	(11.2)	(15.8)	(11.4)	(12.4)
SUMMER	---	---	-24.5	---
	---	---	(13.7)	---
SCHOOL	---	---	-87.0	---
	---	---	(13.9)	---
.				
.				
.				
Sample constraint:	nonsummer weekdays	summer weekdays	weekend days	nonsummer weekdays
$R^2_{adj.}$/s.e.e.	.3249/152.44	.2248/144.70	.3913/160.15	.2978/209.84
no. of obs./ d.f.	925/882	362/320	734/689	925/882

Note: In all regressions involving the set of SKIDS variables, the coefficients for variables SKID630 through SKID634 have been omitted here, for reasons mentioned in the text of this Appendix.

TABLE A-1 (cont.)

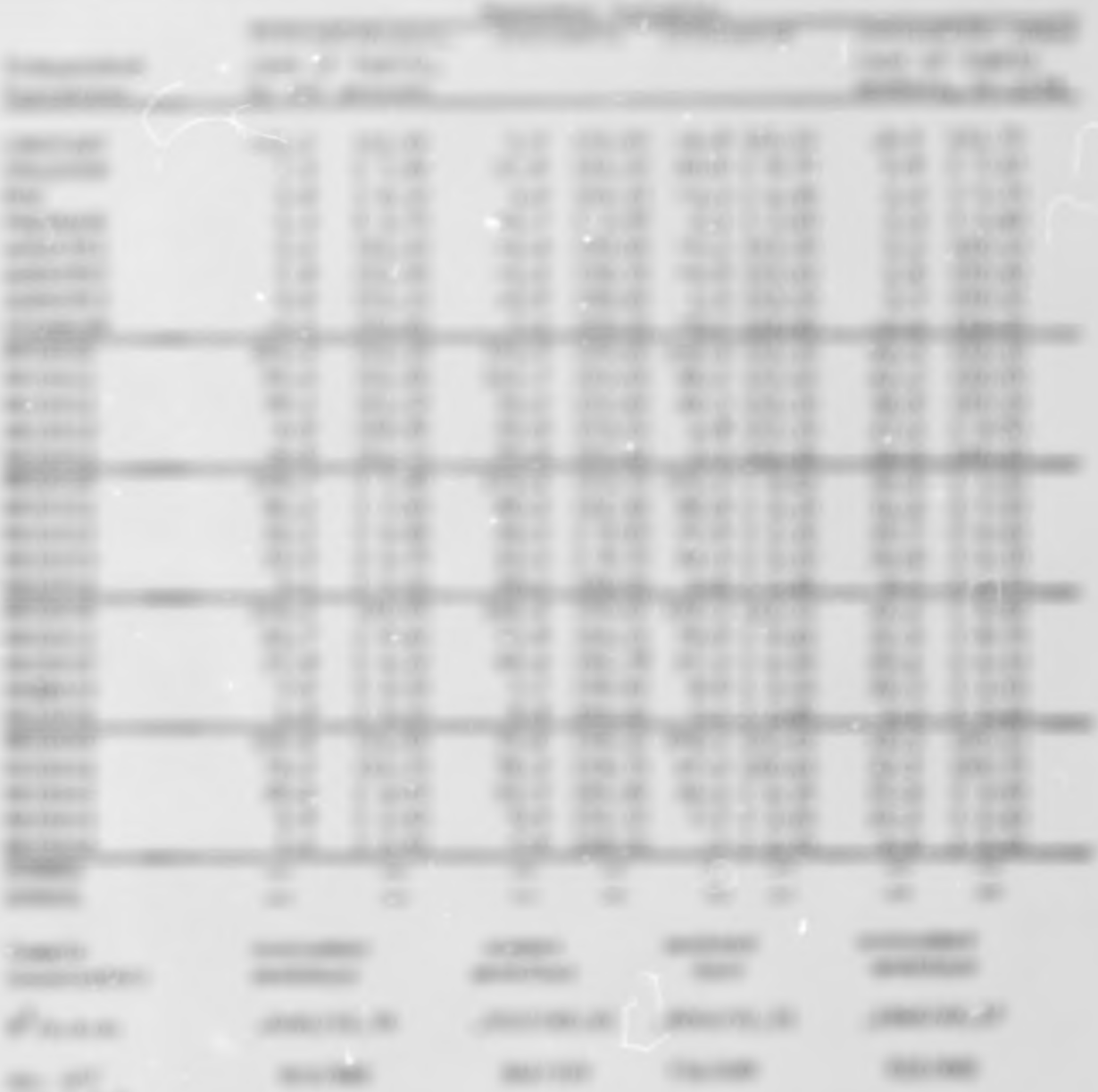
Independent Variable:	Dependent Variable: (5) DHOURS: total home work time, all persons	(6) DHOURS: total home work time, all persons	(7) CAREIV: wife's physical care of family members	(8) CAREIV: wife's physical care of family members
CONSTANT	315.2	267.0	-7.9	-1.9
	(135.2)	(89.8)	(11.7)	(25.9)
COLLEGEW	-114.3	20.0	10.0	9.2
	(98.8)	(51.2)	(7.1)	(11.4)
RSW	0.5	-48.2	4.8	1.3
	(43.4)	(40.1)	(5.7)	(8.3)
PREMAGE	15.5	24.4	.4	-0.4
	(15.9)	(11.7)	(1.6)	(3.1)
AGEVIFE1	57.1	41.7	.5	5.1
	(126.1)	(78.4)	(10.2)	(24.2)
AGEWIFE2	91.0	19.1	5.5	-12.6
	(127.8)	(80.1)	(10.4)	(24.5)
AGEWIFE3	130.1	43.1	4.3	-5.7
	(129.3)	(79.6)	(10.1)	(24.8)
DISABLEW	231.6	-33.4	-6.9	-0.7
	(123.4)	(110.8)	(13.3)	(23.6)
KKID10	142.3	220.7	241.4	125.3
	(87.1)	(72.8)	(10.3)	(14.7)
KKID11	145.8	125.0	88.0	73.4
	(97.0)	(73.2)	(10.1)	(18.6)
KKID12	31.1	140.7	32.4	30.1
	(91.4)	(72.8)	(10.2)	(17.5)
KKID13	80.4	94.8	7.1	16.9
	(94.2)	(72.6)	(8.9)	(19.0)
KKID14	87.5	263.5	-2.0	17.8
	(75.3)	(77.6)	(10.2)	(18.1)
KKID20	188.3	154.3	130.4	142.3
	(58.0)	(51.0)	(7.2)	(11.1)
KKID21	221.2	205.5	77.4	67.0
	(53.8)	(48.1)	(7.2)	(10.3)
KKID22	72.2	114.2	29.2	24.3
	(37.9)	(31.3)	(4.4)	(7.3)
KKID23	87.0	122.6	14.3	12.1
	(41.0)	(31.3)	(4.3)	(7.8)
KKID24	74.4	177.4	4.1	12.7
	(44.3)	(34.6)	(4.7)	(8.5)
KKID30	142.4	287.1	117.4	128.4
	(67.0)	(79.7)	(9.9)	(16.7)
KKID31	95.8	134.2	45.2	71.7
	(68.3)	(56.6)	(8.7)	(13.1)
KKID32	68.1	74.9	26.5	39.5
	(34.5)	(29.0)	(4.1)	(6.6)

TABLE A-1 (cont.)

Independent Variable:	(5) [illegible]: total home work time, all persons	(6) [illegible]: total home work time, all persons	(7) [illegible]: wife's physical care of family members	(8) [illegible]: wife's physical care of family members
[illegible]	114.7	125.4	5.3	5.7
	(25.9)	(29.4)	(3.0)	(4.0)
[illegible]	52.4	102.8	2.2	7.2
	(27.2)	(22.2)	(3.2)	(3.2)
[illegible]	185.8	222.9	13.7	42.7
	(85.5)	(68.8)	(24.2)	(23.2)
[illegible]	111.1	75.7	84.8	44.2
	(78.8)	(42.8)	(18.7)	(23.2)
[illegible]	44.7	74.5	23.8	34.4
	(27.8)	(25.4)	(3.4)	(5.3)
[illegible]	71.2	89.8	4.2	7.6
	(21.2)	(17.4)	(3.4)	(4.2)
[illegible]	53.5	105.2	1.8	4.7
	(34.8)	(21.4)	(3.5)	(4.7)
[illegible]	---	-28.8	---	---
	---	(23.8)	---	---
[illegible]	---	-129.9	---	---
	---	(28.2)	---	---
.				
.				
.				

Sample constraint:	[illegible]	[illegible]	[illegible]	[illegible]
$R^2_{adj.}$/s.e.e.	.2854/215.67	.2115/345.38	.4173/48.23	.3956/10.32
n; df obs./ s.E.	362/320	734/689	825/802	[illegible]

[illegible]



[illegible]

Table A-1 (cont.)

Variable:	Means of Selected Variables for the Three Main Samples: Nonsummer weekday sample	Summer weekday sample	Weekend day sample
NKIDS10	.032	.033	.033
NKIDS11	.036	.025	.033
NKIDS12	.034	.030	.033
NKIDS13	.036	.025	.033
NKIDS14	.032	.033	.033
NKIDS20	.060	.064	.060
NKIDS21	.069	.086	.076
NKIDS22	.158	.166	.158
NKIDS23	.174	.144	.163
NKIDS24	.128	.133	.128
NKIDS30	.028	.028	.023
NKIDS31	.042	.050	.050
NKIDS32	.149	.160	.155
NKIDS33	.234	.354	.266
NKIDS34	.179	.229	.192
NKIDS40	.032	.025	.030
NKIDS41	.024	.033	.034
NKIDS42	.146	.122	.142
NKIDS43	.230	.193	.207
NKIDS44	.104	.135	.127
NKIDS50	.001	.003	.003
NKIDS51	.003	.003	.003
NKIDS52	.013	.019	.011
NKIDS53	.057	.080	.076
NKIDS54	.034	.047	.037
NKIDS60	.009	.003	.005
NKIDS61	.004	.003	.003
NKIDS62	.020	.033	.022
NKIDS63	.043	.069	.040
NKIDS64	.020	.058	.020
NKIDS70	.004	.000	.001
NKIDS71	.004	.000	.003
NKIDS72	.034	.000	.023
NKIDS73	.077	.000	.053
NKIDS74	.053	.000	.040
Number of observations	925	362	734

(As noted at the start of this table, a coding error mismeasured the SUNHOL variable. About half of the Sunday observations were given SUNHOL=0 in the weekend sample, and a similar number of observations has SUNHOL=1 in the weekday samples, even though all observations in the weekend sample truly refer to weekend observations and no weekend observations were included in the weekday sample. The only effect of the coding error was to throw some suspicion on the SUNHOL coefficient and standard error for each model run on the weekend sample.)

samples had too few children under two years of age within families of five or more children. Rather than report coefficients for age-and-parity classes represented by only four or fewer positive observations, I dropped all of the larger-family coefficients from Table A-1.

Also omitted from the list of independent variables, even in the full regressions, were variables relating to the labor force participation of the wife. It might seem natural to include them, in order to be able to ask to what extent a working wife gives less time to each specific household chore than a non-working wife. The influence of the mother's work status on child care is taken up later in this appendix. For present purposes, however, this is an inappropriate specification. The issues of key importance here are the influences of the presence of children and the education of the couple on their time use in and out of the home. Children and the couple's education affect all their time use patterns simultaneously. To quantify their impact on, say, child care time, it is a lot simpler not to include the wife's work status as a competing variable, in order to avoid having to ask, after the regression, to what extent this work status itself is shifted by the presence of children, thereby further shifting the amount of child care time given by the wife. Similar reasoning led me to exclude the wife's preference ratings for different home tasks from the list of independent variables: her stated preferences, like her labor force participation, are simultaneously determined along with her home time use by the couple's background, and including the preference variables would only lead to the further task of asking how responsive these were to the couple's background.

Some of the patterns revealed by the regressions are not surprising. It turns out that virtually all of the housework and virtually all child

care are done by the wife. Husbands, older children, and nonfamily females (primarily babysitters) do help out, especially on weekends and summer weekdays. Yet over half of any indoor task gets done by the wife. This pattern is consistent enough that most of the regressions for time inputs by the husband or hired females yielded such low R^2s, low average time inputs, and low significance of coefficients that they were not worth reporting here.

Some of the patterns regarding the home's less rewarding chores also square with intuition. Meal preparation, meal cleanup, and washing are performed mainly by the wife, and her burden in these areas is raised by pre-teenage children. Teenagers help out enough so that her total time on these chores is raised only 0.6 hours per week as a year-round average by the presence of each teenager. Teenagers in turn put in more home chore time than they seem to add to the mother's total chores--though one can wonder whether their rate of home production per hour matches hers. Regressions were also run for other chores--chauffering, ironing, and regular house care--but too many zeroes were recorded in these time use categories for interesting regression results to show up.

Several clear patterns in child care time emerge from Table A-1. The impact of a child on total care time is much greater for an infant than for an older child. A newborn requires tremendous physical care, a one-year-old requires large amounts of both physical and other (interactive) care, and the demands drop off for each older age group. A glance at the coefficients further suggests that the impact of a child of given age on total care time tends to be lower the more children there are. One of two children in the home seems to affect total care time less than does an only

child, and one of three less than one of two. The pattern does not continue, however, from three-child families to four-child families. Each child in a four-child family seems to have the same or greater impact on total care time as each child of the same age in a three-child family. There is no obvious explanation for this result. The unreported coefficients for larger families do suggest that the net impact of a child of any age above one year (i.e., for any age group well represented in the large-family observations) is lower for five- and six-child families than for four-child families. The prevailing pattern appears, at a glance, to be one of declining impact of one child on total care time the more siblings he or she has. This pattern is examined more carefully below.

Class Differences in Child Care Time?

Past authors have suggested that a wife with more schooling and socio-economic status will tend to put more hours of time into the care of each child. This conclusion was reached, with varying degrees of qualification, by the U.S. Bureau of Human Nutrition and Home Economics (1944), Leibowitz (1972; 1974), Hill and Stafford (1974), Vanek (1973), and Iralai and others (1972). The issue is one that might raise class sentiments, and deserves a closer look.

The literature to date has advanced two related hypotheses about class differences in child care time. The first is that a higher-status wife tends to put more hours into child care per child. The second is that she tends to devote more time to each child than would a lower-status wife having the same number of children. The first hypothesis is correct. The second is incorrect.

The regressions in Table A-1 demonstrate that when one has held the number and ages of children constant, the education of the wife and the status (predicted wage rate) of the husband have a small and insignificant effect on total time put into care of family members. The signs of the coefficients for extra education and status are usually positive, as past authors implied, but the difference between the wife's having a college degree and her having dropped out of high school has less predicted impact on child care time per child than, say, the difference between having two and having three children for given parental characteristics. The unimpressiveness of the effects of the couple's education and status on child care time shows up consistently. It shows up for all times of the week and year. It shows up both for the wife's child care time and for child care time by all persons. It shows up when the current regressions are cross-checked by (unreported) regressions yielding predicted care time imputs of children in samples restricted to high- and then to low-education wives. The Cornell survey data clearly imply, then, that the tendency of higher-status wives to put in more child care time per child is primarily or even entirely due to the fact that they have fewer children on the average. Here, as elsewhere in this book, we encounter the conclusion that the mechanisms governing family size are prime determinants of the inputs that one generation gives to each member of the next.

This finding can be reconciled with the cited findings to the contrary by other authors. Some of the studies noting class differences in child care time were simply based on raw averages too crude for reliable conclusions about class differences. This is true of the studies by Gulbert and others (1967) and by the International team of Szalai and others (1972, pp. 249-263). Authors who took enough care to use regressions based on large

samples often masked the present finding by their choices of variables. Professors Hill and Stafford, for example, used total household work time as a dependent variable, since the Michigan survey they used did not yield any breakdown between child care and other tasks. The apparent extra impact of a child of a more educated and higher-income couple might reflect not differences in time inputs into a child but a greater tendency of low-education wives to find time for the child at the expense of other home chores instead of leisure or paid work. This is especially true since the independent variables used by these authors fail to identify parity effects properly. By using the same variable for the impact of a child of given age whatever the number of his siblings, Hill and Stafford seem to have passed over the point that lower-status wives, having more children and less leisure on the average, might find the time for an extra child more at the expense of other household work, thereby showing a lower impact of the extra child on total household work even if the extra child received the same amount of care time as a higher-status child of the same parity.

Professor Leibowitz, using the better Cornell survey data, chose more appropriate dependent variables than Hill and Stafford had at their disposal. She divided the Syracuse sample into subsamples consisting of families with high-education wives and families with low-education wives. It turned out that the predicted values of care time were higher for the high-education group, though not always significantly so. This result hinged on the fact that the high-education sample had a greater intercept value (care time with no children), even though they had lower apparent impacts of each child on total care time than did the low-education group. In Leibowitz' formulation, as in that of Hill and Stafford, children are presented by the numbers of

there exists each age group, with no recognition of the fact that the impact of an extra child on each age group depends strongly on how many siblings are present. The likelihood that this simplification of the independent variables affected Leibowitz' conclusions is underlined by another property of the Syracuse sample. In that sample it turns out that the families having no children have less schooling on the average than the sample as a whole, while those having one child had higher than average schooling. This explains why the regressions for care time among higher-education families had higher intercepts and higher predicted values for small (and perhaps zero) family sizes: the intercepts and predicted values were inspired up by the relative absence of childless couples and the greater representation of one-child couples. This tainting of the regression line is avoided by making all the presence-of-children variables specific to the total number in the family.

Another study purporting to show marked class differences in child care time is that of the U.S. Bureau of Human Nutrition and Home Economics (1965). That study found that in the late 1950s and early 1960s rural households spent quite a bit less time on child care for each age of the youngest child in the family than did urban clusters of six prestigious eastern women's colleges. This result is hard to interpret, given the nature of the sample and the impossibility of inspecting the original data behind these averages. I strongly suspect that the differences do not relate at all to education, but only to rural-urban differences in the reporting of care used in current tokens. Rural wives may report the large amounts of time spent both supervising the children and working about the house and fail to primarily time spent in things other than child care, while the urban wives record such multiple-use time mainly as child care. It is might be that there are true rural-urban differences in the devoting of time

and average on child care. It is hard to tell, but it does appear that any differences observed in this study tended more on the consideration option than on any option by educational class.

Though past studies have acknowledged the extent to which differences in child care time hinge on differences in flexibility, the present research offered other patterns examined by others. It is still true that a wife with more schooling will tend to put more care into each child if mothers and ages of children are held constant across classes. But the present suggestion is also to reject the hypothesis that children of more educated parents receive markedly less time than children of less-schooled parents. In addition, nothing in table 4-1 serves to confirmed the plausible assertion that a more educated wife is more productive in developing a child's inherent most potential with each hour and each hourly discrimination she spends on him or her. That issue is hard to test, given the multicollinearity between her unobserved characteristics and such influences as the amount of purchased inputs given to the child.

Working Mothers and Child Care

To determine how child care time is influenced by family composition and by the age and education of the parents, it has proved convenient to omit the mother's work status from the list of independent variables, as noted above. Yet it is reasonable to assume how a mother's working for pay outside the home affects the amount of time spent on child care by herself and by others. The movement of mothers into the labor force has been one of the most conspicuous changes in work habits in this century, and the attractive pay prospects for women remain bright enough to make it likely that the trend toward having women and children at the same time will continue, even without

[illegible] [illegible] [illegible] [

To minimize the effects of a student's guess answer on scaled score units, I devised the interpolation of the numbers of wrong (not right) responses:

(a) [illegible] wrong, omitted and nothing[2] answer and the minimum;

(b) [illegible] wrong, omitted and nothing answer and the minimum;

(c) [illegible] wrong, omitted and nothing answer and the minimum; and

(d) [illegible] wrong, omitted and nothing answer and the minimum.

But such interpolated expressions were not on the absence of physical cues and of which were given to [illegible] evidence to the site and to all patterns. These regressions are the same interpolated expressions as those used the Reading Unit [illegible] and [illegible] and [illegible] to the effects of various numbers and kind of children, considered into mind should hereby, see shown in Table [illegible]. The mathematics of these coefficients for the figures of item as evidence mean shown various estimated in illustrated in Figure [illegible], which follows the coefficients from Table [illegible] over the childhood of the children took shown point gains.

The estimates being not scaled estimates in the item dressed in score mark in machine difficulty to the student's scale station. Hence, the point being averages show that nothing between item based children, and other children, from interesting matters, as one would expect. A large area of proceeding item item between with a student's job in item its simple sense of being based children. In terms there, nothing matters are able to provide the same amount of there and item out of interest item item more children[illegible] for [illegible].

Other more methods as nothing of item not points for one another the item as the whole item providing the date of the item [illegible].

[illegible]

Table A-2 (Cont.). Hours of Child-Care Time per Year Associated with Various Numbers and Ages of Children Present in the Home, Working versus Non-working Mothers (based on data from the Cornell Time Use Survey, 1967-68)

E. Total Year-Round Child Care Time	Families with Working Mothers		Families with Non-Working Mothers	
	By Mother	By All Persons	By Mother	By All Persons
For the only child present, aged <1:	a	a	1145.0	1585.0
1:	a	a	854.6	1049.1
2-5:	352.0	713.3	421.1	618.9
6-11:	170.7	375.8	a	a
12-17:	52.0	120.3	109.9	118.3
With 2 children present, for each child aged <1:	810.3	1083.1	994.1	1237.3
1:	552.8	1154.3	630.7	876.8
2-5:	257.8	435.2	293.3	433.9
6-11:	161.7	383.7	192.6	263.3
12-17:	33.0	53.7	99.4	138.2
With 3 children present, for each child aged <1:	a	a	870.4	1018.9
1:	a	a	575.2	685.2
2-5:	234.4	387.5	252.0	367.4
6-11:	117.3	237.5	118.0	202.7
12-17:	28.8	44.5	36.3	64.7
With 4 children present, for each child aged <1:	a	a	668.1	1032.9
1:	a	a	377.0	497.1
2-5:	189.7	366.7	273.8	370.2
6-11:	97.7	117.5	98.5	169.5
12-17:	50.1	82.2	41.1	56.3

Notes:

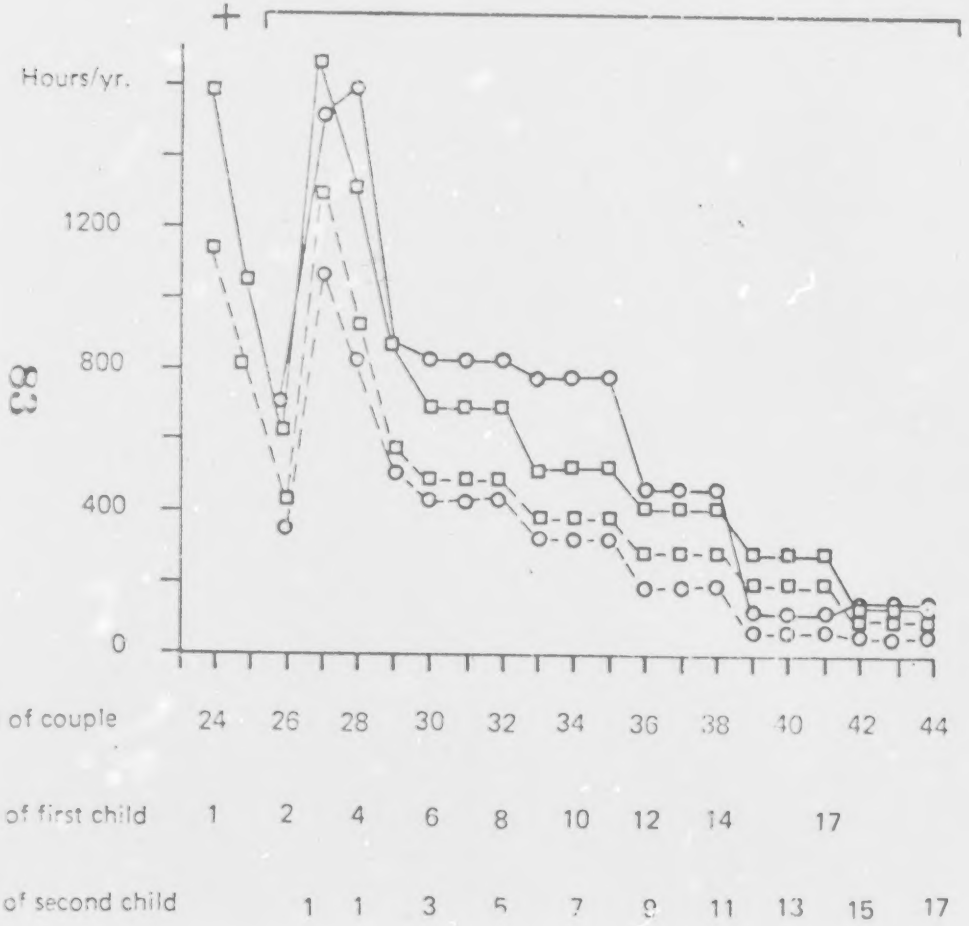
a: figures omitted, since the underlying regression coefficients were based on fewer than 10 children of the appropriate age, family size, and interview day of the week.
b: based on only 10 children in the appropriate category.
c: based on only 14 children in the appropriate category.
d: based on only 12 children in the appropriate category.

The figures are calculated from unreported regressions using the independent variables shown in Table A-1. Each weekday coefficient in those models was multiplied by 4.348 to convert from minutes per weekday to weekday hours per year. Each weekend coefficient was correspondingly multiplied by 1.739. Note that each coefficient is not the same thing as the child-care impact of that one child, as explained in the text of this appendix. Rather each is the contribution of one child that age to the total effect of having that many children (versus having none) in the home. (This distinction is unimportant, of course, for only children.)

Figure A-1 Child Care Time in Hypothetical Two-Child Families, Working versus Nonworking Mother (3-year child spacing)

	Total hours of child care, over the 19 yrs. in parentheses*
care time by all persons, mother works	11,446.6 (11.6 hrs./work.)
care time by all persons, mother does not work	10,528.6 (10.6 hrs./wk.)
care time by nonworking mothers	7,634.5 (7.7 hrs./wk.)
care time by working mothers	5,913.1 (6.0 hrs./wk.)

*(Figures for the first 2 years in the life of the first child could not be compared, since the sample for working mothers included almost no only children under 2 years in age.)



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each individual child, as do non-working mothers. Another means of adjusting to the job is the tendency of working mothers to spend less time at nonchild home tasks, since the effect on child care time is much smaller than the average number of hours worked for pay (25.5 hours a week). Yet it remains true, even when the numbers and ages of children are held constant, that working mothers spend less of their own time on child care. The difference is on the border of statistical significance, though a mammoth sample would probably show clear significance.

The striking result in Table A-2 and Figure A-1, however, is that the reduction in the mother's child care time appears to be more than matched by an increase in the time spent by the husband and others caring for her children. In the two-child case illustrated in Figure A-1 it appears that each hour of contact the children lose with her is matched with about an hour and a half of care from others. This outcome can be interpreted in either of two ways. It could be the result of a reporting bias, as would occur if every single minute of babysitters' time were reported as child care while only part of the time spent by a nonworking mother at home with the children was reported as child care. Alternatively, it may actually be that working mothers spend more of their nonworking hours of the week to child care than nonworking mothers do, in an attempt to avoid shortchanging the children. Either interpretation, though, tends to leave little confirmation of the suspicion that children are deprived by a mother's work. If the reporting bias were responsible for the greater total care hours for children of working mothers, one must recognize that underlying this reporting bias is the fact that much of the time spent by the nonworking mother at home when her children are at home involves no more contact with them than a passive type of baby-

sitting. If, on the other hand, working mothers actually concentrate more of their off-hour attention to their children, the result is the same: we find little hint of a net overall reduction in total child-care time, even when holding the numbers and ages of children constant. The hypothesis of deprivation from a mother's career is not rejected, but it must find evidence not implying a serious reduction in total adult attention.

Estimating Time Inputs Into Siblings

The comments made thus far about the relationship of child care time to numbers and ages of children could be made on the basis of casual inspection of the coefficients in Table A-1. Yet a proper quantification of the child care time received by individual children, and the time costs of individual children, requires a more careful processing of the regression estimates. The coefficients for minutes per day of time spent on different days of the week and seasons of the year first need to be aggregated to yield hours of time use per year. That involves multiplying each figure on minutes per nonsummer weekday by 3.261 [=(365.25/60)x(3/4 of the yr.)x(5/7 of the week)], each figure on minutes per summer weekday by 1.087, and each figure on minutes per weekend day by 1.739. Next, the spacing of births determines which coefficients are relevant for each year of a child's life in appraising his impact on time use. For example, the first of two siblings born two years apart is an only child for his first two years, while the other is an only child for his last two years in the home, assuming they both leave home at the same age. The coefficients for a two-child family should therefore be used only for the years in which they are both present in the home. Another complication to be introduced in processing the estimates of time inputs into children is that the regression coefficient for a given age group and number

of children is not exactly the impact of a child that age on time use in a family achieving that number of children with his presence. Were he not present, that family would be smaller and a different set of child coefficients would be relevant. An example should help clarify this subtlety:

> The net impact on total care time caused in a given year by a three-year-old who is the second of two children is not just the coefficient SKID322, which is associated with a three-year-old in a family with two children present. If the other child is, say six years old, the net impact of the three-year-old is the difference between the predicted value of care time inputs for the family with this pair of children and the predicted value that would obtain if the six-year-old were an only child. This difference, the net impact of the younger child on care time, equals SKID322 + SKID323 - SKID313, which does not exactly equal SKID323. Correspondingly, the net impact of the older child on care time in the same family equals SKID322 + SKID323 - SKID312, which is not exactly equal to SKID323.

The impact of children on various birth orders on total child care time (physical care plus other care) was calculated for different spacing intervals in accordance with these guidelines. The estimates are given in Table 1 in the text above. An examination of the figures there confirms that a child tends to make a greater difference to total care time the fewer in number are his siblings. First-born and last-born children also appear to have greater impacts on total child care time than do middle children, because the first-born and last-born spend part of their childhood as the only child in the household. Again, as with the raw regression coefficients, there appears to be little difference between the care-time use impact of a third versus a fourth child.

Note that the figures in Table 1 refer to the estimated impact of the child on total care time during his presence in the home, and not to the time inputs he himself receives. The net impacts are thus low numbers

where three or four children are present. In some cases, in fact, the values are negative for individual years (interpreted here), implying that the presence of the extra child for such years lowers total care time. The negative values are counter to intuition. They stem from the inability of a less-than-mammoth sample of families to generate coefficients so finely tuned that they imply positive net inputs for all children in all spacing and parity combinations in all years. Some of the negative predicted inputs, at any rate, are as negative as one standard error of estimate.

To convert these inputs on total care time into estimates of the inputs of care time received by an individual sibling, it is necessary to employ the assumptions made above about upper and lower bounds on the inputs of time into a child. The upper bound on the care time he receives is the time he would receive as an only child with similar parents. The lower bound is his input in Table 1. The preferred estimate of the care time he receives is the average of those two extreme estimates. For each sibling position the average can be computed as the average of his column in Table 1 and the only-child column. The individual-year averages are omitted here. At the bottom of the table are presented the raw 10-year sums of the hours put into each child's care according to these mid-range estimates.

The estimates for total care time inputs into children in different family settings support that a child receives more care time by having fewer siblings, by having greater age gaps between siblings, and by being first or last. The ratios shown in Table 7 confirm that an only child receives much more time than a sibling. Either child in a two-child family also receives a good deal more than each child in larger families. Middle children receive less time than first or last children, especially in wider-spaced

families, where the first and last each spend several years as the only child in the household. And the wider the average spacing, the more such child reversions. These patterns are discussed at greater length in the text where they are compared with the apparent effects of sibling position on child achievement.

The estimates in Table 1 imply that a last-born receives more time inputs than a first-born when the average spacing between siblings is three years or wider. This result stems from the ability of the eldest siblings to care for the youngest when the age gaps are wide. What needs to be clarified is how the extent of this care of younger siblings by older siblings was calculated and incorporated into the overall estimates of time inputs. The complicated estimation procedure described above was based on regression estimates of the effects of the presence of each child on the total child care time logged by all persons (in Regressions [III]-[III] and [IV]-[IV] of Table A-1). The use of the all-persons regressions was dictated by the necessity of cutting down on the number of calculations. The more logical procedure of working up separate estimates of the hours of care time by the wife, the husband, older children, and all others was just too time-consuming to be practical. The decision to use the all-persons regressions merely means that part of the net impact of older siblings on child care time by all persons is not really related to the care of him, but instead reflects care by him. It is thus necessary to subtract out estimates of the care of younger siblings by older siblings in order to get a more accurate picture of what time the older siblings are receiving over their childhood. This subtraction was done. Table A-3 reports my estimates of the total amounts of child care

Table 1-5. Predicted Cost of Taxpayer Utility by Urban Individual Income Authority and Spacing of Individuals. Persons of type shown are income owners in the households in the urban individual

Urban Spacing	Individual	Cost of and Taxpayer Utility by			
[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]
Urban	1	$1.21	0	0	0
	2	Auth.	Auth.	Auth.	Auth.
Urban	1	$1.21	0	0	Auth.
	2	[illegible]	$1.21	0	0
	3	$14.28	[illegible]	$1.21	0
Urban	1	$1.21	0	Auth.	Auth.
	2	$1.21	$1.21	0	Auth.
	3	$14.28	$14.28	$1.21	0
	4	$14.28	$14.28	$14.28	$1.21
Urban	1	$1.21	0	Auth.	Auth.
	2	$14.27	$1.21	0	Auth.
	3	$14.27	$14.27	$1.21	0
	4	$14.27	$14.27	$14.27	$1.21

Notes:

Auth. = Not individuals, because the the Council Urbanism supply did not include families with such numbers and spacing of children.

The estimates are based on assumptions concerning utilizing cost that is the numbers of children in each of five age groups and in other variables. It was found that the number of children in each urban age group (birth, 5-17) had no significant effect on the total time spent by each age group in caring for family members. The only significant influences on each item in urban utilization were the numbers of children in each of the preschool age brackets (1-2, 3, 4-5). When the cost in each of the urban utility's five in the households, I applied the appropriate preschool coefficients so that how much utilization families in average in each urban age group. The total number and the urban utilized among the utilities in that agencies. (In the case of the 4-5) as usage, I arbitrarily distributed all of the cost of preschool utilities to those who were 4-5 years old. Each utility's cost item was then summed over the years to arrive at the [illegible] household three ages 4-5).

[illegible]